

The late Prof. Tacchini.

As a tribute to the memory of the late distinguished Italian astronomer, of whom an obituary notice appeared in the columns of NATURE last week, may I be permitted to add a few personal reminiscences? Prof. Tacchini took part in the eclipse expedition of 1875 to the Nicobar Islands. He joined our party from India, where he had been staying from the previous year, having been commissioned by his Government to make observations on the transit of Venus of 1874. The Italian Government sanctioned his remaining in India until the following year in order that he might make use of the opportunity with the instruments in his charge for the observation of the forthcoming total solar eclipse. Of the little band of observers who assembled on the Island of Camorta in April, 1875, most are happily still with us. Vogel, the introducer of "orthochromatic" photography, has passed away, but Pedler, Waterhouse, and others will remember the pleasant *camaraderie* which existed between ourselves and our Italian colleague. The expedition failed in its object through a cloudy sky, and we were all more or less the victims of intermittent malarial fever; but we made the best of adverse circumstances, and under conditions which, to many a party of observers similarly placed, would have been extremely trying, the good understanding which the members had arrived at among themselves helped to lighten the burden of our disappointment. Not the least weighty factor in the formation of this good fellowship among the representatives of different nations was the geniality of Tacchini, with whom we parted on the P. and O. steamer *Baroda* on the homeward voyage with every regret.

April 15.

R. MELDOLA.

Propagation of Earthquake Waves.

MR. RUDZKI, in his letter to NATURE of April 6, observes that "it is only for perfectly elastic and isotropic bodies that the separation of the dilatational (normal) from the tortional (transverse) wave takes place with certainty"; and his conclusion is that "it is more than highly improbable that the effect of internal friction would neutralise the effect of ælotropism." If the term "internal friction" is intended to refer to the effect of pressure, this objection was forestalled by Major Dutton by the remark that "towards this more compact and continuous condition (of a compact mineral substance with a feeble pronounced cleavage), the pressure of great depths in the earth should, it may seem, tend to bring the material subject to it."

To me it is refreshing to learn that any objection can be raised to the view that the two speeds of earthquake waves are respectively condensational and tortional, the latter being held to prove a high degree of rigidity for the interior of the earth.

To examine the question whether the interior is to a considerable depth liquid or solid formed one subject of my "Physics of the Earth's Crust," and I came to the conclusion that it is liquid; and, so far as I am aware, my arguments have never been refuted. On this question Sir A. Geikie writes (NATURE, February 9), "the geological belief rests upon a large body of evidence from the structure of the terrestrial crust, which it is difficult or impossible to explain except on the supposition of an internal mass which, at least in its outer parts, is sufficiently liquid to emerge at the surface as molten lava."

To produce arguments on the opposite side of the question is another matter, and that derived from the two speeds of earthquake propagation is perhaps the strongest. I was consequently led to inquire whether the same result could not be obtained on the hypothesis of a liquid magma holding water gas in solution, subject to Henry's law that the same volume of gas can be absorbed by a given volume of the liquid at all pressures. The result which I obtained was that two waves would be propagated with different velocities, the one a condensational wave depending on the elasticity of the liquid, and the other a wave depending upon the pressure and the volume of the gas which could be held in solution by a given volume of the liquid.

If e be the elasticity of the liquid and D its density,

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then $\sqrt{e/D}$ will be the velocity of the condensational wave. And if P be the pressure and rV the volume of gas which can be held in solution by the volume V of the liquid, then $\sqrt{P/rD}$ will be the velocity of the gaseous wave. If we accept Laplace's law of density, P/D will increase with the depth, and r will probably decrease, hence the velocity of the gaseous wave will increase (*Proc. Cambridge Phil. Soc.*, vol. xii., part v., 1903).

Harlton, Cambridge, April 10.

O. FISHER.

The Ancient Races of the Thebaid.

ON my return to Oxford I saw Prof. Pearson's letter in your issue of March 30.

Since Prof. Pearson admits that he is not an anatomist, it would serve no useful purpose to discuss with him the anatomical value of the criteria which Mr. MacIver and I employed in our analysis of the skulls of the ancient inhabitants of the Theban province of Egypt.

The letter may be regarded as an interesting record of a method of interpreting percentage values adopted by a professed statistician.

ARTHUR THOMSON.

Oxford, April 8.

THERE is an old saying that all good science is short-hand common sense. I am sorry that Prof. Arthur Thomson does not think it worth his while in the case of his just published far-reaching negroid cranial criterion to convert the esoteric methods of the anatomist into simple language for the benefit of other readers of NATURE, if not for that of the "professed statistician." I hope he will meet me later when I ask him to discuss, as I propose shortly to do, the mathematico-statistical treatment of his volume, which is of a somewhat remarkable character. Meanwhile, in order to expedite those further investigations by professed craniologists which his discovery is exciting, it would be of great value if he would tell us to what negro series he, *a priori*, applied his criteria, and what percentages of pure negroid, non-negroid, and intermediate crania he found in that series.

KARL PEARSON.

Inversions of Temperature on Ben Nevis.

THE recent letters of Mr. Dines and Mr. Rotch (NATURE, February 16 and March 30) have suggested that a note as to the occurrence of temperature inversions on Ben Nevis may be of interest.

During the thirteen years 1891-1903, occasions were not infrequent when the temperature at the top of the mountain (4406 feet) was higher than that at the base. These inversions have been grouped according as the summit temperature was the higher, (1) at one hour at least of the day; (2) at each of the twenty-four hours of the day; (3) on the mean of the twenty-four hours of the day.

The total number of cases in the thirteen years was as follows:—

	Class I.	Class II.	Class III.
January	7	—	3
February	18	1	5
March	11	—	1
April	9	—	—
May	7	—	—
June	8	—	—
July	4	—	—
August	4	—	—
September	22	—	3
October	15	—	5
November	29	3	8
December	24	5	8
Year	158	9	33

Thus inversions occurred at all seasons, but inversions continued throughout the twenty-four hours of the civil day only in February, November, and December, and those of Class III. only between September and March. The average difference of temperature between Ben Nevis and Fort William ranged from $16^{\circ}8$ F. in April to $14^{\circ}4$ in December, the mean for the whole year being $15^{\circ}4$. Hence inversions were at all seasons large departures from the usual conditions.

The greatest inversion was recorded during the great frost of February, 1895, when at 9 a.m. on February 19 the summit was $17^{\circ}.6$ warmer than the base (Ben Nevis $33^{\circ}.6$, Fort William $16^{\circ}.0$). The longest continued inversion occurred during November 2-5, 1897, when the summit temperature was the higher for fifty-eight consecutive hours, the mean daily temperature on November 4 being $9^{\circ}.7$ higher on Ben Nevis than at Fort William.

The Ben Nevis observations, of course, afford a comparison only between the conditions at the summit and those at the base of the mountain. It is more than probable that on many occasions when the summit temperature becomes nearly, though not quite, as high as that at the base, there is an inversion of temperature in part of the air-column between the summit and sea-level.

ANDREW WATT.

Scottish Meteorological Society, Edinburgh, April 12.

Stanton Drew.

THE mysteries of this group of circles—the next in importance to those of Avebury and Stonehenge—are not yet fully unveiled, even by the very remarkable astronomical discoveries made in them by Sir Norman Lockyer or by his interesting description of them.

The diameter of the north-east circle is 97 English feet, or 100 of an old Mediterranean foot of 11.64 inches. This is within an inch or two of the diameter of the outer sarsen ring at Stonehenge, which is in itself a very significant fact. The diameters of the south-western and central circles are respectively 150 and 380 of this old foot, so that the diameters of the circles (within a very slight working error) are in proportion one to the other of 5, $7\frac{1}{2}$, and 19, the latter being the Metonic cycle number.

The distances between the various parts of the group, subject to a working error of from $\frac{1}{2}$ to $\frac{3}{4}$ of 1 per cent. only, are:—

Centre of cove through great circle to centre of north-east circle = 14 diameters of north-east circle.

Centre of great circle to Hauteville's Quoit = 5 diameters of the great circle, or 19 diameters of the north-east circle, the latter being the Metonic cycle number.

Centre of south-west circle through great circle to Hauteville's Quoit = 7 diameters of the great circle.

Centre of great circle to two stones too far to the west to be shown on the plan in NATURE = 9 diameters of the great circle.

With the exception of the last, anyone can test these proportionate distances by the plan given in NATURE, but who will tell us what was the meaning or object of them?

A. L. LEWIS.

ALCOHOL IN INDUSTRY.

THE committee, consisting of Sir Henry Primrose, K.C.B. (chairman), Sir W. Holland, M.P., Mr. J. Scott-Montagu, M.P., Sir William Crookes, Mr. Lothian Nicholson, Dr. Somerville, of the Board of Agriculture, Dr. Thorpe, the director of the Government Laboratories, and Mr. Thomas Tyrer, appointed last autumn by the Chancellor of the Exchequer to inquire into the use of duty-free alcohol in the arts and manufactures have got together their evidence and published their report with commendable promptitude. The report, we are glad to find, is unanimous, and this unanimity has doubtless not been without its influence in accelerating the business of the committee and the appearance of their report.

The subject, as was to be anticipated, has not been without its difficulties, for, as the committee state, a duty that yields more than twenty millions a year is a public interest that cannot be trifled with; but, as usual when men are determined to find a solution, it is remarkable how purely academic difficulties tend to disappear. Now that the suggestions of the committee are before us, the wonder is that they should not have been given effect to a quarter of a century

ago. We are afraid the delay does not reflect creditably upon the enterprise, energy, or constructive ability of the numerous groups of manufacturers who are interested in obtaining the greatest possible facilities in the use of duty-free alcohol in the arts. This attitude of *laissez-faire* is seen, and commented upon by the committee, in connection with the apathy and general ignorance of manufacturers with respect to the provisions of Section 8 of the Finance Act of 1902, which gave the commissioners of Inland Revenue large discretionary powers as regards the use of spirit for industrial purposes. The committee point out that advantage has not been taken of the Act to the extent that might have been anticipated, and they have been surprised to find in examining the witnesses sent by the various Chambers of Commerce, who certainly ought to have had official knowledge of its existence, how very inadequate has been their acquaintance with its provisions.

In view of this general indifference one is tempted to inquire whether the manufacturers have had any real grievance, since they have made so little individual or collective effort to remove it. There is certainly no evidence that any collective effort has been made in the past, or, if it had been made, that the Treasury or the Revenue authorities would not have sympathised with it. The Exchequer, at all events since 1855, when the present system of denaturing spirit came into existence, may be said to have disclaimed any idea of collecting a revenue on alcohol used solely as a raw material and for purely industrial purposes. If the hitherto existing system of denaturing and control had proved so irksome that the development of chemical industry was impossible, it might have been supposed that Parliament would have been troubled with the question long ago. But as an actual fact the languid interest of the chemical manufacturers needed, apparently, to be supplemented by the quickening influence of the internal-combustion engine, and the possible applications of spirit as a motor-fuel supplied to a jaded House of Commons engaged in the discussion of a Finance Bill that stimulus which was necessary to secure from the Chancellor the promise of the departmental inquiry, which it would seem the great body of manufacturing chemists was too lukewarm to ask for.

Great cry has been made in the past that the hindrances to a free and untrammelled supply of alcohol have cost us the coal-tar dye industry, which originated in this country, and at one time flourished here; but the committee apparently have had little difficulty in ascertaining how "little wool" there is in this cry. They say they are satisfied that the assertion, as a statement of historical fact, is destitute of substantial foundation. In their opinion the main cause which led to the decadence of the industry in this country is that which we have repeatedly insisted on in these columns, viz. the failure of those responsible for the management and for the finance of the industry here during the years 1860-1880 to realise the vital importance of its scientific side, and their consequent omission to provide adequately for its development on that side.

It is true, however, that after signing the report, the two Members of Parliament named were induced to modify their assent to the unanimous finding of the committee as to the real cause of the decline of the coal-tar dye industry in this country. It will be interesting to see from the evidence, when this is published, what support Sir William Holland and Mr. John Scott-Montagu are able to find for the view they express in their letter to the Chancellor.

In reality, "alcohol" plays a very small part in